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SCIENCE

A WEEKLY JOURNAL DEVOTED TO THE ADVANCEMENT OF SCIENCE, PUBLISHING THE
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FRIDAY, DECEMBER 6, 1901.

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MSS. intended for publication and books, etc., intended for review should be sent to the responsible editor, Professor J. McKeen Cattell, Garrison-on-Hudson, N. Y.

HENRY AUGUSTUS ROWLAND.*

IN reviewing the scientific work of Professor Rowland one is most impressed by its originality. In quantity, as measured by printed page or catalogue of titles, it has been exceeded by many of his contemporaries; in quality it is equaled by that of only a very, very small group. The entire collection of his important papers does not exceed thirty or forty in number and his unimportant papers were few. When, at the unprecedently early age of thirty-three years, he was elected to membership in the National Academy of Sciences, the list of his published contributions to science did not contain over a dozen titles, but any one of not less than a half dozen of these, including what may properly be called his very first original investigation, was of such quality as to fully entitle him to the distinction then conferred.

Fortunately for him, and for science as well, he lived during a period of almost unparalleled intellectual activity, and his work was done during the last quarter of that century to which we shall long turn with admiration and wonder. During these twenty-five years the number of industrious cultivators of his own favorite field increased enormously, due in large measure to the

* Address by Dr. T. C. Mendenhall at the Memorial Meeting at the Johns Hopkins University on October 16.

stimulating effect of his own enthusiasm, and while there was only here and there one possessed of the *divine afflatus* of true genius, there were many ready to labor most assiduously in fostering the growth, development and final fruition of germs which genius stopped only to plant. A proper estimate of the magnitude and extent of Rowland's work would require, therefore, a careful examination, analytical and historical, of the entire mass of contributions to physical science during the past twenty-five years, many of his own being fundamental in character and far-reaching in their influence upon the trend of thought, in theory and in practice. But it was quality, not quantity, that he himself most esteemed in any performance; it was quality that always commanded his admiration or excited him to keenest criticism; no one recognized more quickly than he a real gem, however minute or fragmentary it might be, and by quality rather than by quantity we prefer to judge his work to-day, as he would himself have chosen.

Rowland's first contribution to the literature of science took the form of a letter to *The Scientific American*, written in the early autumn of 1865, when he was not yet seventeen years old. Much to his surprise, this letter was printed, for he says of it, 'I wrote it as a kind of joke and did not expect them to publish it.' Neither its humor nor its sense, in which it was not lacking, seems to have been appreciated by the editor, for by the admission of certain typographical errors he practically destroyed both. The embryo physicist got nothing but a little quiet amusement out of this, but in a letter of that day he declares his intention of some time writing a sensible article for the journal that so unexpectedly printed what he meant to be otherwise. This resolution he seems not to have forgotten, for nearly six years later there appeared in its columns what was, as far as is known, his

second printed paper and his first serious public discussion of a scientific question.

It was a keen criticism of an invention which necessarily involved the idea of perpetual motion, in direct conflict with the great law of the conservation of energy which Rowland had already grasped. It was, as might be expected, thoroughly well done and received not a little complimentary notice in other journals. This was in 1871, the year following that in which he was graduated as a civil engineer from the Rensselaer Polytechnic Institute, and the article was written while in the field at work on a preliminary railroad survey. A year later, having returned to the Institute as instructor in physics, he published in the *Journal of the Franklin Institute* an article entitled, 'Illustrations of Resonances and Actions of a Similar Nature,' in which he described and discussed various examples of resonance or 'sympathetic' vibration. This paper, in a way, marks his admission to the ranks of professional students of science and may be properly considered as his first formal contribution to scientific literature; his last was an exhaustive article on spectroscopy, a subject of which he, above all others, was master, prepared for a new edition of the *Encyclopædia Britannica*, not yet published.

Early in 1873 the *American Journal of Science* printed a brief note by Rowland on the spectrum of the Aurora, sent in response to a kindly and always appreciated letter from Professor George F. Barker, one of the editors of that journal. It is interesting as marking the beginning of his optical work. For a year, or perhaps for several years, previous to this time, however, he had been busily engaged on what proved to be, in its influence upon his future career, the most important work of his life. To climb the ladder of reputation and success by simple, easy steps might have contented Rowland, but it would have been quite out of har-

mony with his bold spirit, his extraordinary power of analysis and his quick recognition of the relation of things. By the aid of apparatus entirely of his own construction and by methods of his own devising, he had made an investigation, both theoretical and experimental, of the magnetic permeability and the maximum magnetization of iron, steel and nickel, a subject in which he had been interested in his boyhood.

On June 9, 1873, in a letter to his sister, he says: "I have just sent off the results of my experiments to the publisher and expect considerable from it; not, however, filthy lucre, but good, substantial reputation." What he did get from it, at first, was only disappointment and discouragement. It was more than once rejected because it was not understood and finally he ventured to send it to Clerk Maxwell, in England, by whose keen insight and profound knowledge of the subject it was instantly recognized and appraised at its full value. Regretting that the temporary suspension of meetings made it impossible for him to present the paper at once to the Royal Society, Maxwell said he would do the next best thing, which was to send it to the *Philosophical Magazine* for immediate publication, and in that journal it appeared in August, 1873, Maxwell himself having corrected the proofs to avoid delay. The importance of the paper was promptly recognized by European physicists, and abroad, if not at home, Rowland at once took high rank as an investigator.

In this research he unquestionably anticipated all others in the discovery and announcement of the beautifully simple law of the magnetic circuit, the magnetic analogue of Ohm's law, and thus laid the foundation for the accurate measurement and study of magnetic permeability, the importance of which, both in theory and in practice during recent years, it is difficult to

overestimate. It has always seemed to me that when consideration is given to his age, his training, and the conditions under which his work was done, this early paper gives a better measure of Rowland's genius than almost any performance of his riper years. During the next year or two he continued to work along the same lines in Troy, publishing not many, but occasional, additions to and developments of his first magnetic research. There was also a paper in which he discussed Kohlrausch's determination of the absolute value of the Siemens unit of electrical resistance, foreshadowing the important part which he was to play in later years in the final establishment of standards for electrical measurement.

In 1875, having been appointed to the professorship of physics in Johns Hopkins University, the faculty of which was just then being organized, he visited Europe, spending the better part of a year in the various centers of scientific activity, including several months at Berlin in the laboratory of the greatest continental physicist of his time, von Helmholtz. While there he made a very important investigation of the magnetic effect of moving electrostatic charges, a question of first rank in theoretical interest and significance. His manner of planning and executing this research made a marked impression upon the distinguished director of the laboratory in which it was done and, indeed, upon all who had any relations with Rowland during its progress. He found what von Helmholtz himself had sought for in vain, and when the investigation was finished in a time which seemed incredibly short to his more deliberate and painstaking associates, the director not only paid it the compliment of an immediate presentation to the Berlin Academy, but voluntarily met all expenses connected with its execution.

The publication of this research added much to Rowland's rapidly growing reputation, and because of that fact, as well as on account of its intrinsic value, it is important to note that his conclusions have been held in question, with varying degrees of confidence, from the day of their announcement to the present. The experiment is one of great difficulty and the effect to be looked for is very small, and therefore likely to be lost among unrecognized instrumental and observational errors. It was characteristic of Rowland's genius that with comparatively crude apparatus he got at the truth of the thing in the very start. Others who have attempted to repeat his work have not been uniformly successful, some of them obtaining a wholly negative result, even when using apparatus apparently more complete and effective than that first employed by Rowland.

Such was the experience of Lecher in 1884, but in 1888 Roentgen confirmed Rowland's experiments, detecting the existence of the alleged effect. The result seeming to be in doubt, Rowland himself, assisted by Hutchinson, in 1889, took it up again, using essentially his original method, but employing more elaborate and sensitive apparatus. They not only confirmed the early experiments, but were able to show that the results were in tolerably close agreement with computed values. The repetition of the experiment by Himstedt in the same year resulted in the same way, but in 1897 the genuineness of the phenomenon was again called in question by a series of experiments made at the suggestion of Lippman, who had proposed a study of the reciprocal of the Rowland effect, according to which variations of a magnetic field should produce a movement of an electrostatically charged body. This investigation, carried out by Crémieu, gave an absolutely negative result, and because the method was entirely different from that employed by Row-

land and, therefore, unlikely to be subject to the same systematic errors, it naturally had much weight with those who doubted his original conclusions.

Realizing the necessity for additional evidence in corroboration of his views, in the fall of the year 1900 the problem was again attacked in his own laboratory, and he had the satisfaction, only a short time before his death, of seeing a complete confirmation of the results he had announced a quarter of a century earlier, concerning which, however, there had never been the slightest doubt in his own mind. It is a further satisfaction to his friends to know that a very recent investigation at the Jefferson Physical Laboratory of Harvard University, in which Rowland's methods were modified so as to meet effectively the objections made by his critics, has resulted in a complete verification of his conclusions.

On his return from Europe, in 1876, his time was much occupied with the beginning of the active duties of his professorship, and especially in putting in order the equipment of the laboratory over which he was to preside, much of which he had ordered while in Europe. In its arrangement great (many of his friends thought undue) prominence was given to the workshop, its machinery, tools, and especially the men who were to be employed in it. He planned wisely, however, for he meant to see to it that much, perhaps most, of the work under his direction should be in the nature of original investigation, for the successful execution of which a well manned and equipped workshop is worth more than a storehouse of apparatus already designed and used by others.

He shortly found leisure, however, to plan an elaborate research upon the mechanical equivalent of heat, and to design and supervise the construction of the necessary apparatus for a determination of the numerical value of this most important

physical constant, which he determined should be exhaustive in character and, for some time to come at least, definitive. While this work lacked the elements of originality and boldness of inception by which many of his principal researches are characterized, it was none the less important. While doing over again what others had done before him, he meant to do it, and did do it, on a scale and in a way not before attempted. It was one of the *great* constants of nature, and, besides, the experiment was one surrounded by difficulties so many and so great that few possessed the courage to undertake it with the deliberate expectation of greatly excelling anything before accomplished. These things made it attractive to Rowland.

The overthrow of the materialistic theory of heat, accompanied as it was by the experimental proof of its real nature, namely, that it is essentially molecular energy, laid the foundation for one of those two great generalizations in science which will ever constitute the glory of the nineteenth century. The mechanical equivalent of heat, the number of units of work necessary to raise one pound of water one degree in temperature, has, with much reason, been called the Golden Number of that century. Its determination was begun by an American, Count Rumford, and finished by Rowland, nearly a hundred years later. In principle the method of Rowland was essentially that of Rumford. The first determination was, as we now know, in error by nearly 40 per cent.; the last is probably accurate within a small fraction of 1 per cent. Rumford began the work in the ordnance foundry of the Elector of Bavaria at Munich, converting mechanical energy into heat by means of a blunt boring tool in a cannon surrounded by a definite quantity of water, the rise in temperature of which could be measured. Rowland finished it in an establishment founded for

and dedicated to the increase and diffusion of knowledge, aided by all the resources and refinements in measurement which a hundred years of exact science had made possible.

As the mechanical theory of heat was the germ out of which grew the principle of the conservation of energy, an exact determination of the relation of work and heat was necessary to a rigorous proof of that principle, and Joule, of Manchester, to whom belongs more of the credit for this proof than to any other one man or, perhaps, to all others put together, experimented on the mechanical equivalent of heat for more than forty years. He employed various methods, finally recurring to the early method of heating water by friction, improving on Rumford's device by creating friction in the water itself. Joule's last experiments were made in 1878, and most of Rowland's work was done in the year following. It excelled that of Joule, not only in the magnitude of the quantities to be observed, but especially in the greater attention given to the matter of thermometry. In common with Joule and other previous investigators, he made use of mercury thermometers, but this was only for convenience, and they were constantly compared with an air thermometer, the results being finally reduced to the absolute scale. By experimenting with water at different initial temperatures he obtained slightly different values for the mechanical equivalent of heat, thus establishing beyond question the variability of the specific heat of water. Indeed, so carefully and accurately was the experiment worked out that he was able to draw the variation curve and to show the existence of a minimum value at 30 degrees C.

This elaborate and painstaking research, which is now classical, was everywhere awarded high praise. It was published in full by the American Academy of Arts and

Sciences by the aid of a fund originally established by Count Rumford, and in 1881 it was crowned as a prize essay by the Venetian Institute. Its conclusions have stood the test of twenty years of comparison and criticism.

In the meantime, Rowland's interest had been drawn, largely, perhaps, through his association with his then colleague, Professor Hastings, towards the study of light. He was an early and able exponent of Maxwell's magnetic theory, and he published important theoretical discussions of electromagnetic action. Recognizing the paramount importance of the spectrum as a key to the solution of problems in ether physics, he set about improving the methods by which it was produced and studied, and was thus led into what will probably always be regarded as his highest scientific achievement.

At that time the almost universally prevailing method of studying the spectrum was by means of a prism or a train of prisms. But the prismatic spectrum is abnormal, depending for its character largely upon the material made use of. The normal spectrum as produced by a grating of fine wires or a close ruling of fine lines on a plane reflecting or transparent surface had been known for nearly a hundred years, and the colors produced by scratches on polished surfaces were noted by Robert Boyle, more than two hundred years ago. Thomas Young had correctly explained the phenomenon according to the undulatory theory of light, and gratings of fine wire and, later, of rulings on glass, were used by Fraunhofer, who made the first great study of the dark lines of the solar spectrum. Imperfect as these gratings were, Fraunhofer succeeded in making with them some remarkably good measures of the length of light-waves, and it was everywhere admitted that for the most precise spectrum measurements they were indispensable. In their

construction, however, there were certain mechanical difficulties which seemed for a time to be insuperable. There was no special trouble in ruling lines as close together as need be; indeed, Nobert, who was long the most successful maker of ruled gratings, had succeeded in putting as many as a hundred thousand in the space of a single inch. The real difficulty was in the lack of uniformity of spacing, and on uniformity depended the perfection and purity of the spectrum produced. Nobert jealously guarded his machine and method of ruling gratings as a trade secret, a precaution hardly worth taking, for before many years the best gratings in the world were made in the United States.

More than thirty years ago an amateur astronomer, in New York City, a lawyer by profession, Lewis M. Rutherford, became interested in the subject and built a ruling engine of his own design. In this machine the motion of the plate on which the lines were ruled was produced at first by a somewhat complicated set of levers, for which a carefully made screw was afterwards substituted. Aided by the skill and patience of his mechanician, Chapman, Rutherford continued to improve the construction of his machine until he was able to produce gratings on glass and on speculum metal far superior to any made in Europe. The best of them, however, were still faulty in respect to uniformity of spacing, and it was impossible to cover a space exceeding two or three square inches in a satisfactory manner. When Rowland took up the problem, he saw, as, indeed, others had seen before him, that the dominating element of a ruling machine was the screw by means of which the plate or cutting tool was moved along. The ruled grating would repeat all of the irregularities of this screw, and would be good or bad just as these were few or many. The problem was, then, to make a screw which would be practically

free from periodic and other errors, and upon this problem a vast amount of thought and experiment had already been expended.

Rowland's solution of it was characteristic of his genius; there were no easy advances through a series of experiments in which success and failure mingled in varying proportions; 'fire and fall back' was an order which he neither gave nor obeyed, capture by storm being more to his mind. He was by nature a mechanician of the highest type, and he was not long in devising a method for removing the irregularities of a screw, which astonished everybody by its simplicity and by the all but absolute perfection of its results. Indeed, the very first screw made by this process ranks today as the most perfect in the world. But such an engine as this might only be worked up to its highest efficiency under the most favorable physical conditions, and in its installation and use the most careful attention was given to the elimination of errors due to variation of temperature, earth tremors and other disturbances. Not content, however, with perfecting the machinery by which gratings were ruled, Rowland proceeded to improve the form of the grating itself, making the capital discovery of the concave grating, by means of which a large part of the complex and otherwise troublesome optical accessories to the diffraction spectroscope might be dispensed with. Calling to his aid the wonderful skill of Brashear in making and polishing plane and concave surfaces, as well as the ingenuity and patience of Schneider, for so many years his intelligent and loyal assistant at the lathe and work-bench, he began the manufacture and distribution, all too slowly for the anxious demands of the scientific world, of those beautifully simple instruments of precision which have contributed so much to the advance of physical science during the past twenty years.

While willing and anxious to give the

widest possible distribution to these gratings, thus giving everywhere a new impetus to optical research, Rowland meant that the principal spoils of the victory should be his, and to this end he constructed a diffraction spectrometer of extraordinary dimensions and began his classical researches on the solar spectrum. Finding photography to be the best means of reproducing the delicate spectral lines shown by the concave grating, he became at once an ardent student and, shortly, a master of that art. The outcome of this was that wonderful 'Photographic Map of the Normal Solar Spectrum,' prepared by the use of concave gratings six inches in diameter and twenty-one and a half feet radius, which is recognized as a standard everywhere in the world. As a natural supplement to this he directed an elaborate investigation of absolute wave-lengths, undertaking to give, finally, the wave-length of not only every line of the solar spectrum, but also of the bright lines of the principal elements, and a large part of this monumental task is already completed, mostly by Rowland's pupils and in his laboratory.

Time will not allow further expositions of the important consequences of his invention of the ruling engine and the concave grating. Indeed, the limitations to which I must submit compel the omission of even brief mention of many interesting and valuable investigations relating to other subjects begun and finished during these years of activity in optical research, many of them by Rowland himself and many of them by his pupils, working out his suggestions and constantly stimulated by his enthusiasm. A list of titles of papers emanating from the Physical Laboratory of the Johns Hopkins University during this period would show somewhat of the great intellectual fertility which its director inspired, and would show, especially, his continued interest in magnetism and electricity, leading to his

important investigations relating to electric units and to his appointment as one of the United States delegates at important international conventions for the better determination and definition of these units. In 1883 a committee appointed by the Electrical Congress of 1881, of which Rowland was a member, adopted 106 centimeters as the length of the mercury column equivalent to the absolute ohm, but this was done against his protest, for his own measurements showed that this was too small by about three-tenths of one per cent. His judgment was confirmed by the Chamber of Delegates of the International Congress of 1893, of which Rowland was himself president, and by which definitive values were given to a system of international units.

Rowland's interest in applied science cannot be passed over, for it was constantly showing itself, often, perhaps, unbidden, an unconscious bursting forth of that strong engineering instinct which was born in him, to which he often referred in familiar discourse and which would unquestionably have brought him great success and distinction had he allowed it to direct the course of his life. Although everywhere looked upon as one of the foremost exponents of pure science, his ability as an engineer received frequent recognition in his appointment as expert and counsel in some of the most important engineering operations in the latter part of the century. He was an inventor, and might easily have taken first rank as such had he chosen to devote himself to that sort of work. During the last few years of his life he was much occupied with the study of alternating electric currents and their application to a system of rapid telegraphy of his own invention. A year ago his system received the award of a grand prix at the Paris Exposition, and only a few weeks after his death the daily papers published cablegrams

from Berlin announcing its complete success as tested between Berlin and Hamburg, and also the intention of the German Postal Department to make extensive use of it.

But behind Rowland, the profound scholar and original investigator, the engineer, mechanician and inventor, was Rowland the man, and any estimate of his influence in promoting the interests of physical science during the last quarter of the nineteenth century would be quite inadequate if not made from that point of view. Born at Honesdale, Pennsylvania, on November 27, 1848, he had the misfortune, at the age of eleven years, to lose his father by death. This loss was made good, as far as it is possible to do so, by the loving care of mother and sisters during the years of his boyhood and youthful manhood. From his father he inherited his love for scientific study, which from the very first seems to have dominated all his aspirations, directing and controlling most of his thoughts. His father, grandfather and great-grandfather were all clergymen and graduates of Yale College. His father, who is described as one 'interested in chemistry and natural philosophy, a lover of nature and a successful trout-fisherman,' had felt, in his early youth, some of the desires and ambitions that afterward determined the career of his distinguished son, but yielding, no doubt, to the influence of family tradition and desire, he followed the lead of his ancestors.

It is not unlikely, and it would not have been unreasonable, that similar hopes were entertained in regard to the future of young Henry, and his preparatory-school work was arranged with this in view. Before being sent away from home, however, he had quite given himself up to chemical experiments, glass-blowing and other similar occupations, and the members of his family were often summoned by the enthusiastic boy to listen to lectures which were fully

illustrated by experiments, not always free from prospective danger. His spare change was invested in copper wire and the like, and his first five-dollar bill brought him, to his infinite delight, a small galvanic battery. The sheets of the *New York Observer*, a treasured family newspaper, he converted into a huge hot-air balloon, which, to the astonishment of his family and friends, made a brilliant ascent and flight, coming to rest, at last, and in flames, on the roof of a neighboring house, and resulting in the calling out of the entire fire department of the town. When urged by his boy friends to hide himself from the rather threatening consequences of his first experiment in aeronautics, he courageously marched himself to the place where his balloon had fallen, saying, 'No! I will go and see what damage I have done.'

When a little more than sixteen years old, in the spring of 1865, he was sent to Phillips Academy at Andover, to be fitted for entering the academic course at Yale. His time there was given entirely to the study of Latin and Greek, and he was in every way out of harmony with his environment. He seems to have quickly and thoroughly appreciated this fact, and his very first letter from Andover is a cry for relief. '*Oh, take me home!*' is the boyish scrawl covering the last page of that letter, on another of which he says, 'It is simply horrible; I can never get on here.' It was not that he could not learn Latin and Greek if he was so minded, but that he had long ago become wholly absorbed in the love of nature and in the study of nature's laws, and the whole situation was to his ambitious spirit most artificial and irksome. Time did not soften his feelings or lessen his desire to escape from such uncongenial surroundings, and, at his own request, Dr. Farrand, principal of the academy at New Jersey, to which city the family had recently removed, was consulted as to what ought to

be done. Fortunately for everybody, his advice was that the boy ought to be allowed to follow his bent, and, at his own suggestion, he was sent, in the autumn of that year, to the Rensselaer Polytechnic Institute at Troy, where he remained five years, and from which he was graduated as a civil engineer in 1870.

It is unnecessary to say that this change was joyfully welcomed by young Rowland. At Andover the only opportunity that had offered for the exercise of his skill as a mechanic was in the construction of a somewhat complicated device by means of which he outwitted some of his schoolmates in an early attempt to haze him, and in this he took no little pride. At Troy he gave loose rein to his ardent desires, and his career in science may almost be said to begin with his entrance upon his work there and before he was seventeen years old.

He made immediate use of the opportunities afforded in Troy and its neighborhood for the examination of machinery and manufacturing processes, and one of his earliest letters to his friends contained a clear and detailed description of the operation of making railroad iron, the rolls, shears, saws and other special machines being represented in uncommonly well executed pen drawings. One can easily see in this letter a full confirmation of a statement that he occasionally made later in life, namely, that he had never seen a machine, however complicated it might be, whose working he could not at once comprehend. In another letter, written within a few weeks of his arrival in Troy, he shows in a remarkable way his power of going to the root of things which even at that early age was sufficiently in evidence to mark him for future distinction as a natural philosopher. On the river he saw two boats equipped with steam pumps, engaged in trying to raise a half-sunken canal boat by pumping the water out of it.

He described engines, pumps, etc., in much detail, and adds, "But there was one thing that I did not like about it; they had the end of their discharge pipe about ten feet above the water, so that they had to overcome a pressure of about five pounds to the square inch to raise the water so high, and yet they let it go after they got it there, whereas if they had attached a pipe to the end of the discharge pipe and let it hang down into the water, the pressure of water on that pipe would just have balanced the five pounds to the square inch in the other, so that they could have used larger pumps with the same engines and thus have got more water out in a given time."

The facilities for learning physics, in his day, at the Rensselaer Polytechnic Institute were none of the best, a fact which is made the subject of keen criticism in his home correspondence, but he made the most of whatever was available and created opportunity where it was lacking. The use of a turning-lathe and a few tools being allowed, he spent all of his leisure in designing and constructing physical apparatus of various kinds with which he experimented continually. All of his spare money goes into this and he is always wishing he had more. While he pays without grumbling his share of the expense of a class supper, he cannot help declaring that 'it is an *awful* price for one night's pleasure; why, it would buy another galvanic battery.' During these early years his pastime was the study of magnetism and electricity, and his lack of money for the purchase of insulated wire for electromagnetic apparatus led him to the invention of a method of winding naked copper wire, which was later patented by some one else and made much of. Within six months of his entering the Institute he had made a delicate balance, a galvanometer and an electrometer, besides a small induction coil and several minor pieces. A few weeks later he announces the finishing

of a Ruhmkorff coil of considerable power, a source of much delight to him and to his friends.

In December, 1866, he began the construction of a small but elaborately designed steam engine which ran perfectly when completed and furnished power for his experiments. A year later he is full of enthusiasm over an investigation which he wishes to undertake to explain the production of electricity when water comes in contact with red-hot iron, which he attributes to the decomposition of a part of the water. Along with all this and much more he maintains a good standing in his regular work in the Institute, in some of which he is naturally the leader. He occasionally writes: 'I am head of my class in mathematics,' or 'I lead the class in natural philosophy,' but official records show that he was now and then 'conditioned' in subjects in which he had no special interest. As early as 1868, before his twentieth birthday, he decided that he must devote his life to science. While not doubting his ability 'to make an excellent engineer,' as he declares, he decides against engineering, saying: "You know that from a child I have been extremely fond of experiment; this liking, instead of decreasing, has gradually grown upon me until it has become a part of my nature, and it would be folly for me to attempt to give it up; and I don't see any reason why I should wish it unless it be avarice, for I never expect to be a rich man. I intend to devote myself hereafter to *science*. If she gives me wealth I will receive it as coming from a friend, but if not, I will not murmur."

He realized that his opportunity for the pursuit of science was in becoming a teacher, but no opening in this direction presenting itself, he spent the first year after graduation in the field as a civil engineer. This was followed by a not very inspiring experience as instructor in natural science in

a western college, where he acquired, however, experience and useful discipline.

In the spring of 1872 he returned to Troy as instructor in physics, on a salary the amount of which he made conditional on the purchase by the Institute of a certain number of hundreds of dollars' worth of physical apparatus. If they failed in this, as afterward happened, his pay was to be greater, and he strictly held them to the contract. His three years at Troy as instructor and assistant professor were busy, fruitful years. In addition to his regular work he did an enormous amount of study, purchasing for that purpose the most recent and most advanced books on mathematics and physics. He built his electro-dynamometer and carried out his first great research. As already stated, this quickly brought him reputation in Europe and, what he prized quite as highly, the personal friendship of Maxwell, whose ardent admirer and champion he remained to the end of his life. In April, 1875, he wrote, "It will not be very long before my reputation reaches this country," and he hoped that this would bring him opportunity to devote more of his time and energy to original research.

This opportunity for which he so much longed was nearer at hand than he imagined. Among the members of the Visiting Board at the West Point Military Academy in June, 1875, was one to whom had come the splendid conception of what was to be at once a revelation and a revolution in methods of higher education. In selecting the first faculty for an institution of learning which, within a single decade, was to set the pace for real university work in America, and whose influence was to be felt in every school and college of the land before the end of the first quarter of a century, Dr. Gilman was guided by an instinct which more than all else insured the success of the new enter-

prise. A few words about Rowland from Professor Michie, of the Military Academy, led to his being called to West Point by telegraph, and on the banks of the Hudson these two walked and talked, 'he telling me,' Dr. Gilman has said, 'his dreams for science, and I telling him my dreams for higher education.' Rowland, with characteristic frankness, writes of this interview, 'Professor Gilman was very much pleased with me,' which, indeed, was the simple truth. The engagement was quickly made. Rowland was sent to Europe to study laboratories and purchase apparatus, and the rest is history, already told and everywhere known.

Rowland's personality was in many respects remarkable. Tall, erect and lithe in figure, fond of athletic sports, there was upon his face a certain look of severity which was, in a way, an index of the exacting standard he set for himself and others. It did not conceal, however, what was, after all, his most striking characteristic, namely, a perfectly frank, open and simple straightforwardness in thought, in speech and in action. His love of truth held him in supreme control, and, like Galileo, he had no patience with those who try to make things appear otherwise than as they actually are. His criticisms of the work of others were keen and merciless, and sometimes there remained a sting of which he himself had not the slightest suspicion. 'I would not have done it for the world,' he once said to me after being told that his pitiless criticism of a scientific paper had wounded the feelings of its author. As a matter of fact, he was warm-hearted and generous, and his occasionally seeming otherwise was due to the complete separation, in his own mind, of the product and the personality of the author. He possessed that rare power, habit in his case, of seeing himself, not as others see him, but as he saw others. He looked at himself and his own work exactly

as if he had been another person, and this gave rise to a frankness of expression regarding his own performance which sometimes impressed strangers unpleasantly, but which, to his friends, was one of his most charming qualities.

Much of his success as an investigator was due to a firm confidence in his own powers, and in the unerring course of the logic of science which inspired him to cling tenaciously to an idea when once he had given it a place in his mind. At a meeting of the National Academy of Sciences in the early days of our knowledge of electric generators he read a paper relating to the fundamental principles of the dynamo. A gentleman who had had large experience with the practical working of dynamos listened to the paper, and at the end said to the academy that unfortunately practice directly contradicted Professor Rowland's theory, to which instantly replied Rowland, 'So much the worse for the practice,' which, indeed, turned out to be the case.

Like all men of real genius, he had phenomenal capacity for concentration of thought and effort. Of this, one who was long and intimately associated with him remarks, "I can remember cases when he appeared as if drugged from mere inability to recall his mind from the pursuit of all-absorbing problems, and he had a triumphant joy in intellectual achievement such as we would look for in other men only from the gratification of an elemental passion." So completely consumed was he by fires of his own kindling that he often failed to give due attention to the work of others, and some of his public utterances give evidence of this curious neglect of the historic side of his subject.

As a teacher his position was quite unique. Unfit for the ordinary routine work of the class room, he taught, as more men ought to teach, by example rather than by precept. Says one of his most eminent

pupils, "Even of the more advanced students, only those who were able to brook severe and searching criticism reaped the full benefit of being under him, but he contributed that which in a university is above all teaching of routine, the spectacle of scientific work thoroughly done and the example of a lofty ideal."

Returning home about twenty years ago, after an expatriation of several years, and wishing to put myself in touch with the development of methods of instruction in physics, and especially in the equipment of physical laboratories, I visited Rowland very soon after, as it happened, the making of his first successful negative of the solar spectrum. That he was completely absorbed in his success was quite evident, but he also seemed anxious to give me such information as I sought. I questioned him as to the number of men who were to work in his laboratory, and although the college year had already begun he appeared to be unable to give even an approximate answer. 'And what will you do with them?' I said. 'Do with them?' he replied, raising the still dripping negative so as to get a better light through its delicate tracings, 'Do with them?—*I shall neglect them.*' The whole situation was intensely characteristic, revealing him as one to whom the work of a drill-master was impossible, but ready to lead those who would be led and could follow. To be neglected by Rowland was often, indeed, more stimulating and inspiring than the closest personal supervision of men lacking his genius and magnetic fervor.

In the fulness of his powers, recognized as America's greatest physicist, and one of a very small group of the world's most eminent, he died on April 16, 1901, from a disease, the relentless progress of which he had realized for several years and opposed with a splendid but quiet courage.

It was Rowland's good fortune to receive

recognition during his life in the bestowal of degrees by higher institutions of learning; in election to membership in nearly all scientific societies worthy of note in Europe and America; in being made the recipient of medals of honor awarded by these societies, and in the generously expressed words of his distinguished contemporaries. It will be many years, however, before full measure can be had of his influence in promoting the interests of physical science, for with his own brilliant career, sufficient of itself to excite our profound admiration, must be considered that of a host of other younger men who lighted their torches at his flame and who will reflect honor upon him whose loss they now mourn, by passing on something of his unquenchable enthusiasm, something of his high regard for pure intellectuality, something of his love of truth and his sweetness of character and disposition.

T. C. MENDENHALL.

*REPORT OF THE BOARD OF VISITORS TO
THE NAVAL OBSERVATORY FOR
THE YEAR 1901.*

IN pursuance to a call issued by the Secretary of the Navy, a meeting of the Board of Visitors to the Naval Observatory was held in Washington, beginning April 9, 1901. The Board was organized by the selection of Charles A. Young as chairman and Ormond Stone as secretary. Another meeting was held in Washington, beginning October 29, 1901. At both of these meetings, and afterwards by correspondence, as careful an examination as time permitted was made, directly and by committee, of the condition and needs of the Observatory, and of such other matters as are referred to in the law creating the Board. In this examination the Board was greatly aided by conferences with the Secretary of the Navy, the Superintendent and staff of the Naval Observatory, officers of the Civil Service

Commission, and others, all of whom have given the Board their most cheerful assistance. As a result of the deliberations of the Board the following recommendations are respectfully offered for consideration:

ASTRONOMICAL DIRECTOR.

It is recommended that no astronomical director be appointed at present, as a dual headship has been found to work unsatisfactorily, and under the existing law the appointment of an astronomer as sole director of the Observatory—which the Board considers the proper solution of the question—is impracticable.

METHOD OF FILLING VACANCIES.

Vacancies should not be filled among assistant astronomers nor among professors of mathematics in the Navy without examination for each vacancy occurring. For example, the results of a given examination should not be used for filling a subsequent vacancy, except in so far as such results may properly form a part of a new independent examination. No distinction should be made between employees of the Observatory and other applicants. Employees should, however, of course, be at liberty to present evidence of experience or capacity as shown by their work at the Observatory in the same manner as other candidates present similar evidence as shown by their work elsewhere. The responsibilities of the positions of assistant astronomer and professor of mathematics are distinctly different from those of a computer, although much of the required experience may properly be gained in connection with the latter position and be credited in the examinations for the higher positions. At the same time it is important that the positions of computer should be filled by persons whose prime interest is in practical and theoretical astronomy and whose ambition it will be to occupy higher positions in the Observatory. As far as is